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THE HISTORY OF ANTHROPOLOGY.*

I HAVE been asked to speak on the history of anthropology. The task that has been allotted to me is so vast and the time at my disposal is so short, that it will be impossible to do justice to the work of the minds that have made anthropology what it is. It would even be futile to characterize the work of the greatest among the contributors to our science. All that I can undertake to do is to discuss the general conditions of scientific thought that have given rise to anthropology.

Viewing my task from this standpoint, you will pardon me if I do not first attempt to define what anthropology ought to be, and with what subjects it ought to deal, but if I take my cue rather from what it is, and how it has developed.

Before I enter into my subject I will say that the speculative anthropology of the 18th and of the early part of the 19th century is distinct in its scope and method from the science which is called anthropology at the present time and is not included in our discussion.

At the present time anthropologists occupy themselves with problems relating to the physical and mental life of mankind as found in varying forms of society, from the earliest times up to the present period, and in all parts of the world. Their researches bear upon the form and functions of the body as well as upon all kinds of manifestations of mental life. Accordingly, the subject matter of anthropology is partly a branch of biology, partly a branch of the mental sciences. Among the mental phenomena language, invention, art, religion, social organization and law have received particular attention. Among anthropologists of our time we find a consid-

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erable amount of specialization of the subject matter of their researches according to the divisions here given.

As in other sciences whose subject matter is the actual distribution of phenomena and their causal relation, we find in anthropology two distinct methods of research and aims of investigation: the one, the historical method, which endeavors to reconstruct the actual history of mankind; the other, the generalizing method, which attempts to establish the laws of its development. According to the personal inclination of the investigator, the one or the other method prevails in his researches. A considerable amount of geographical and historical specialization has also taken place among what may be called the historical school of anthropologists. Some devote their energies to the elucidation of the earliest history of mankind, while others study the inhabitants of remote regions, and still others the survivals of early times that persist in our midst.

The conditions thus outlined are the result of a long development, the beginnings of which during the second half of the 18th century may be clearly observed. The interest in the customs and appearance of the inhabitants of distant lands is, of course, much older. The descriptions of Herodotus show that even among the nations of antiquity, notwithstanding their self-centered civilization, this interest was not lacking. The travelers of the Middle Ages excited the curiosity of their contemporaries by the recital of their experiences. The literature of the Spanish conquest of America is replete with remarks on the customs of the natives of the New World. But there is hardly any indication of the thought that these observations might be made the subject of scientific treatment. They were and remained curiosities. It was only when their relation to our own civilization became the subject of inquiry that the foundations of anthropology were laid. Its germs may be discovered in the early considerations of theologists regarding the relations between pagan religions and the revelations of Christianity. They were led to the conclusion that the lower forms of culture, more particularly of religion, were due to degeneration, to a falling away from the revealed truth, of which traces are to be found in primitive beliefs.

During the second half of the eighteenth century we find the fundamental concept of anthropology well formulated by the rationalists who preceded the French Revolution. The deep-seated feeling that political and social inequality was the result of a faulty development of civilization and that originally all men were born equal, led Rousseau to the naïve assumption of an ideal natural state which we ought to try to regain. These ideas were shared by many and the relation of the culture of primitive man to our civilization remained the topic of discussion. To this period belong Herder's 'Ideen zur Geschichte der Menschheit,' in which perhaps for the first time the fundamental thought of the development of the culture of mankind as a whole is clearly expressed.

About this time Cook made his memorable voyages and the culture of the tribes of the Pacific Islands became first known to Europe. His observations and the descriptions of Forster were eagerly taken up by students and were extensively used in support of their theories. Nevertheless even the best attempts of this period were essentially speculative and deductive, for the rigid inductive method had hardly begun to be understood in the domain of natural sciences, much less in that of the mental sciences.

While, on the whole, the study of the mental life of mankind had in its beginning decidedly a historical character, and while knowledge of the evolution of civilization

was recognized as its ultimate aim, the biological side of anthropology developed in an entirely different manner. It owes its origin to the great zoologists of the eighteenth century, and in conformity with the general systematic tendencies of the times, the main efforts were directed towards a classification of the races of man and to the discovery of valid characteristics by means of which the races could be described as varieties of one species or as distinct species. The attempts at classification were numerous, but no new point of view was developed.

During the nineteenth century a certain approach between these two directions was made, which may be exemplified by the work of Klemm. The classificatory aspect was combined with the historical one and the leading discussion related to the discovery of mental differences between the zoological varieties or races of men, and to the question of polygenism and monogenism. The passions that were aroused by the practical and ethical aspects of the slavery question did much to concentrate attention on this phase of the anthropological problem.

As stated before, most of the data of anthropology had been collected by travelers whose prime object was geographical discoveries. For this reason the collected material soon demanded the attention of geographers, who viewed it from a new standpoint. To them the relations between man and nature were of prime importance and their attention was directed less to psychological questions than to those relating to the dependence of the form of culture upon geographical surroundings, and the control of natural conditions gained by man with the advance of civilization.

Thus we find about the middle of the nineteenth century the beginnings of anthropology laid from three distinct points of view: the historical, the classificatory and the geographical. About this time the historical aspect of the phenomena of nature took hold of the minds of investigators in the whole domain of science. Beginning with biology, and principally through Darwin's powerful influence, it gradually revolutionized the whole method of natural and mental science and led to a new formulation of their problems. The idea that the phenomena of the present have developed from previous forms with which they are genetically connected and which determine them, shook the foundations of the old principles of classification and knit together groups of facts that hitherto had seemed disconnected. Once clearly enunciated, the historical view of the natural sciences proved irresistible and the old problems faded away before the new attempts to discover the history of evolution. From the very beginning there has been a strong tendency to combine with the historical aspect a subjective valuation of the various phases of development, the present serving as a standard of comparison. The oft-observed change from simple forms to more complex forms, from uniformity to diversity, was interpreted as a change from the less valuable to the more valuable and thus the historical view assumed in many cases an ill-concealed teleological tinge. The grand picture of nature in which for the first time the universe appears as a unit of ever-changing form and color, each momentary aspect being determined by the past moment and determining the coming changes, is still obscured by a subjective element, emotional in its sources, which leads us to ascribe the highest value to that which is near and dear to us.

The new historical view also came into conflict with the generalizing method of science. It was imposed upon that older view of nature in which the discovery of general laws was considered the ultimate aim of investigation. According to this

view laws may be exemplified by individual events, which, however, lose their specific interest once the laws are discovered. The actual event possesses no scientific value in itself, but only so far as it leads to the discovery of a general law. This view is, of course, fundamentally opposed to the purely historical view. Here the laws of nature are recognized in each individual event, and the chief interest centers in the event as an incident of the picture of the world. In a way the historic view contains a strong, esthetic element, which finds its satisfaction in the clear conception of the individual event. It is easily intelligible that the combination of these two standpoints led to the subordination of the historical fact under the concept of the law of nature. Indeed, we find all the sciences which took up the historical standpoint for the first time, soon engaged in endeavors to discover the laws according to which evolution has taken place. The regularity in the processes of evolution became the center of attraction even before the processes of evolution had been observed and understood. All sciences were equally guilty of premature theories of evolution based on observed homologies and supposed similarities. The theories had to be revised again and again, as the slow progress of empirical knowledge of the data of evolution proved their fallacy.

Anthropology also felt the quickening impulse of the historic point of view, and its development followed the same lines that may be observed in the history of the other sciences. The unity of civilization and of primitive culture that had been divined by Herder now shone forth as a certainty. The multiplicity and diversity of curious customs and beliefs appeared as early steps in the evolution of civilization from simple forms of culture. The striking similarity between the customs of remote districts was the proof of the uniform manner in which civilization had developed the world over. The laws according to which this uniform development of culture took place became the new problem which engrossed the attention of anthropologists.

This is the source from which sprang the ambitious system of Herbert Spencer and the ingenious theories of Edward Burnett Tylor. The underlying thought of the numerous attempts to systematize the whole range of social phenomena or one or the other of its features—such as religious belief, social organization, forms of marriage—has been the belief that one definite system can be found according to which all culture has developed, that there is one type of evolution from a primitive form to the highest civilization which is applicable to the whole of mankind, that notwithstanding many variations caused by local and historical conditions, the general type of evolution is the same everywhere.

This theory has been discussed most clearly by Tylor, who finds proof for it in the sameness of customs and beliefs the world over. The typical similarity and the occurrence of certain customs in definite combinations are explained by him as due to their belonging to a certain stage in the development of civilization. They do not disappear suddenly, but persist for a time in the form of survivals. These are, therefore, wherever they occur, a proof that a lower stage of culture of which these customs are characteristic has been passed through.

Anthropology owes its very existence to the stimulus given by these scholars and to the conclusions reached by them. What had been a chaos of facts appeared now marshaled in orderly array, and the great steps in the slow advance from savagery to civilization were drawn for the first time with a firm hand. We can not overestimate the influence of the bold general-

izations made by these pioneers of modern anthropology. They applied with vigor and unswerving courage the new principles of historical evolution to all the phenomena of civilized life, and in doing so sowed the seeds of the anthropological spirit in the minds of historians and philosophers. Anthropology, which was hardly beginning to be a science, ceased at the same time to lose its character of being a single science, but became a method applicable to all the mental sciences and indispensable to all of them. We are still in the midst of this development. The sciences first to feel the influence of anthropological thought were those of law and religion. But it was not long before ethics, esthetics, literature and philosophy in general were led to accept the evolutionary standpoint in the particular form given to it by the early anthropologists.

The generalized view of the evolution of culture in all its different phases which is the final result of this method may be subjected to a further analysis regarding the psychic causes which bring about the regular sequence of the stages of culture. Owing to the abstract form of the results, this analysis must be deductive. It can not be an induction from empirical psychological data. In this fact lies one of the weaknesses of the method which led a number of anthropologists to a somewhat different statement of the problem. I mention here particularly Adolf Bastian and Georg Gerland. Both were impressed by the sameness of the fundamental traits of culture the world over. Bastian saw in their sameness an effect of the sameness of the human mind and terms these fundamental traits 'Elementargedanken,' declining all further consideration of their origin, since an inductive treatment of this problem is impossible. For him the essential problem of anthropology is the discovery of the elementary ideas, and in further pursuit of the inquiry, their modification under the influence of geographical environment. Gerland's views agree with those of Bastian in the emphasis laid upon the influence of geographical environment on the forms of culture. In place of the mystic elementary idea of Bastian, Gerland assumes that the elements found in many remote parts of the world are a common inheritance from an early stage of cultural development. It will be seen that in both these views the system of evolution plays a secondary part only, and that the main stress is laid on the causes which bring about modifications of the fundamental and identical traits. There is a close connection between this direction of anthropology and the old geographical school. Here the psychic and environmental relations remain amenable to inductive treatment, while, on the other hand, the fundamental hypotheses exclude the origin of the common traits from further investigation.

The subjective valuation which is characteristic of most evolutionary systems, was from the very beginning part and parcel of evolutionary anthropology. It is but natural that in the study of the history of culture our own civilization should become the standard, that the achievements of other times and other races should be measured by our own achievements. In no case is it more difficult to lay aside the 'Culturbrille'—to use Von den Steinen's apt term—than in viewing our own culture. For this reason the literature of anthropology abounds in attempts to define a number of stages of culture leading from simple forms to the present civilization, from savagery through barbarism to civilization, or from an assumed presavagery through the same stages to enlightenment.

The endeavor to establish a schematic line of evolution naturally led back to new attempts at classification in which each group bears a genetic relation to the other.

Such attempts have been made from both the cultural and the biological point of view.

It is necessary to speak here of one line of anthropological research that we have hitherto disregarded. I mean the linguistic method. The origin of language was one of the much discussed problems of the nineteeth century, and owing to its relation to the development of culture, it has a direct anthropological bearing. The intimate ties between language and ethnic psychology were expressed by no one more clearly than by Steinthal, who perceived that the form of thought is molded by the whole social environment of which language is part. Owing to the rapid change of language, the historical treatment of the linguistic problem had developed long before the historic aspect of the natural sciences was understood. The genetic relationship of languages was clearly recognized when the genetic relationship of species was hardly thought of. With the increasing knowledge of languages they were grouped according to common descent, and when no further relationship could be proved, a classification according to morphology was attempted. To the linguist whose whole attention is directed to the study of the expression of thought by language, language is the individuality of a people, and therefore a classification of languages must present itself to him as a classification of peoples. No other manifestation of the mental life of man can be classified so minutely and definitely as language. In none are the genetic relations more clearly established. It is only when no further genetic and morphological relationship can be found, that the linguist is compelled to coordinate languages and can give no further clue regarding their relationship and origin. No wonder, then, that this method was used to classify mankind, although in reality the linguist classified only languages. The result of the classification seems eminently satisfactory on account of its definiteness as compared with the results of biological and cultural classifications.

Meanwhile the methodical resources of biological or somatic anthropology had also developed and had enabled the investigator to make nicer distinctions between human types than he had been able to make. The landmark in the development of this branch of anthropology has been the introduction of the metric method, which owes its first strong development to Quetelet. A little later we shall have to refer to this subject again. For the present it may suffice to say that a clearer definition of the terms 'type' and 'variability' led to the application of the statistical method by means of which comparatively slight varieties can be distinguished satisfactorily. By the application of this method it soon became apparent that the races of man could be subdivided into types which were characteristic of definite geographical areas and of the people inhabiting them. The same misinterpretation developed here as was found among the linguists. As they identified language and people, so the anatomists identified somatic type and people and based their classifications of peoples wholly on their somatic characters.

The two principles were soon found to clash. Peoples genetically connected by language, or even the same in language, were found to be diverse in type, and people of the same type were found to be diverse in language. Furthermore, the results of classifications according to cultural groups disagreed with both the linguistic and the somatic classifications. In long and bitter controversies the representatives of these three directions of anthropological research contended for the correctness of their conclusions. This war of opinions was fought out particularly on the ground

of the so-called Aryan question, and only gradually did the fact come to be understood that each of these classifications is the reflection of a certain group of facts. The linguistic classification records the historical fates of languages and indirectly of the people speaking these languages; the somatic classification records the blood relationships of groups of people and thus traces another phase of their history; while the cultural classification records historical events of still another character, the diffusion of culture from one people to another and the absorption of one culture by another. Thus it became clear that the attempted classifications were expressions of historical data bearing upon the unwritten history of races and peoples, and recorded their descent, mixture of blood, changes of language and development of culture. Attempts at generalized classifications based on these methods can claim validity only for that group of phenomena to which the method applies. An agreement of their results, that is, original association between somatic type, language and culture, must not be expected. Thus the historical view of anthropology received support from the struggles between these three methods of classification.

We remarked before that the evolutionary method was based essentially on the observation of the sameness of cultural traits the world over. On the one hand, the sameness was assumed as proof of a regular, uniform evolution of culture. On the other hand, it was assumed to represent the elementary idea which arises by necessity in the mind of man and which can not be analyzed, or as the earliest surviving form of human thought.

The significance of these elementary ideas or universal traits of culture has been brought into prominence by the long continued controversy between the theory of their independent origin and that of their transmission from one part of the world to another. This struggle began even before the birth of modern anthropology, with the contest between Grimm's theory of the origin and history of myths and Benfey's proof of transmissions, which was based on his learned investigations into the literary history of tales. It is still in progress. On the one hand, there are investigators who would exclude the consideration of transmission altogether, who believe it to be unlikely and deem the alleged proof irrelevant, and who ascribe sameness of cultural traits wholly to the psychic unity of mankind and to the uniform reaction of the human mind upon the same stimulus. An extremist in this direction was the late Daniel G. Brinton. On the other hand, Friedrich Ratzel, whose recent loss we lament, inclined decidedly to the opinion that all sameness of cultural traits must be accounted for by transmission, no matter how far distant the regions in which they are found. In comparison with these two views the third one, which was mentioned before as represented by Gerland, namely, that such cultural traits are vestiges or survivals of the earliest stages of a generalized human culture, has found few supporters.

It is evident that this fundamental question can not be settled by the continued discussion of general facts, since the various explanations are logically equally probable. It requires actual investigation into the individual history of such customs to discover the causes of their present distribution.

Here is the place to mention the studies in folklore which have excited considerable interest in recent times and which must be considered a branch of anthropological research. Beginning with records of curious superstitions and customs and of popular tales, folklore has become the science of all the manifestations of popular life.

Folklorists occupy themselves primarily with the folklore of Europe and thus supplement the material collected by anthropologists in foreign lands. The theorists of folklore are also divided into the two camps of the adherents of the psychological theory and those of the historical theory. In England the former holds sway, while on the continent the historical theory seems to be gaining ground. The identity of the contents of folklore all over Europe seems to be an established fact. To the one party the occurrence of these forms of folklore seems to be due in part to psychic necessity, in part to the survival of earlier customs and beliefs. To the other party it seems to owe its origin to the spread of ideas over the whole continent which may, in part at least, be followed by literary evidence.

However this controversy, both in folklore and in anthropology, may be settled, it is clear that it must lead to detailed historical investigations, by means of which definite problems may be solved, and that it will furthermore lead to psychological researches into the conditions of transmission, adaptation and invention. Thus this controversy will carry us beyond the limits set by the theory of elementary ideas, and by that of a single system of evolution of civilization.

Another aspect of the theories here discussed deserves special mention. I mean the assumption of a 'folk psychology' (Völkerpsychologie) as distinct from individual psychology. Folk psychology deals with those psychic actions which take place in each individual as a social unit, and the psychology of the individual must be interpreted by the data of a social psychology, because each individual can think, feel and act only as a member of the social group to which he belongs. The growth of language and all ethnic phenomena have thus been treated from the point of view of a social psychology, and special attention has been given to the subconscious influences which sway crowds and masses of people, and to the processes of imitation. I mention Steinthal, Wundt, Baldwin, Tarde, Stoll, among the men who have devoted their energies to these and related problems. Notwithstanding their efforts, and those of a number of sociologists and geographers, the relation of 'folk psychology' to individual psychology has not been elucidated satisfactorily.

We will now turn to a consideration of the recent history of somatology. The historical point of view wrought deep changes also in this branch of anthropology. In place of classification the evolution of human types became the main object of investigation. The two questions of man's place in nature and of the evolution of human races and types came to the front. The morphological and embryological methods which had been developed by biologists were applied to the human species and the new endeavors were directed to the discovery of the predecessor of man, to his position in the animal series, and to evidences regarding the direction in which the species develops. I need mention only Huxley and Wiedersheim to characterize the trend of these researches.

In one respect, however, the study of the human species differs from that of the animal series. I stated before that the slight differences between types which are important to the anthropologist had led to the substitution of the metric quantitative description for the verbal or qualitative method. The study of the effects of natural selection, of environment, of heredity, as applied to man, made the elaboration of these methods a necessity. Our interest in slight differences is so much greater in man than in animals or plants, that here the needs of quantitative precision were first felt. We owe it to Francis Galton that the methods of the quantitative study of the

varieties of man have been developed and that the study has been extended from the field of anatomy over that of physiology and experimental psychology. His researches were extended and systematized by Karl Pearson, in whose hands the question which was originally one of the precise treatment of the biological problem of anthropology has outgrown its original limits and has become a general biological method for the study of the characteristics and of the development of varieties.

We may now summarize the fundamental problems which give to anthropology its present character. In the biological branch we have the problem of the morphological evolution of man and that of the development of varieties. Inseparable from these questions is also that of correlation between somatic and mental characters which has a practical as well as a theoretical interest. In psychological anthropology the important questions are the discovery of a system of the evolution of culture, the study of the modifications of simple general traits under the influence of different geographical and social conditions, the question of transmission and spontaneous origin, and that of folk psychology *versus* individual psychology. It will, of course, be understood that this enumeration is not exhaustive, but includes only some of the most important points of view that occupy the minds of investigators.

The work of those students who are engaged in gathering the material from which this history of mankind is to be built up is deeply influenced by these problems. It would be vain to attempt to give even the briefest review of what has been achieved by the modest collector of facts, how his efforts have covered the remotest parts of the world, how he has tried to uncover and interpret the remains left by the races of the past.

I think we may say, without injustice, that his work is directed principally to the explanation of special problems that derive their chief interest from a personal love for the particular question and an ardent desire to see its obscurity removed and to present its picture in clear outlines. Nevertheless the well trained and truly scientific observer will always be aware of the general relations of his special problem and will be influenced in his treatment of the special question by the general theoretical discussions of his times. It must be said with regret that the number of anthropological observers who have a sufficient understanding of the problems of the day is small. Still their number has increased considerably during the last twenty years and consequently a constant improvement in the reliability and thoroughness of the available observations may be noticed.

One or two aspects of the research work of the field anthropologist must be mentioned. The studies in prehistoric archæology have been given a lasting impulse by the discussions relating to the evolution of mankind and of human culture. Two great problems have occupied the attention of archæologists, the origin and first appearance of the human race, and the historical sequence of races and of types of culture. To the archæologist the determination of the chronological order is an important one. The determination of the geological period in which man appeared, the chronological relation of the earliest types of man to their later successors, the sequence of types of culture as determined by the artifacts of each period, and approximate determinations of the absolute time to which these remains belong are the fundamental problems with which archæology is concerned. The results obtained have the most immediate bearing upon the general question of the evolution of culture, since the ideal aim of archæology practically coincides with this general problem, the solution of which

would be contained in a knowledge of the chronological development of culture. Of course, in many cases the chronological question can not be answered and then the archæological observations simply rank with ethnological observations of primitive people.

The field work of ethnologists has been influenced in several directions by the theoretical discussions of anthropologists. We do not need to dwell on the fact that the scope of ethnological research has become more extensive and exhaustive by taking into consideration more thoroughly than before the whole range of cultural phenomena. More interesting than this is the stimulus that has been given to historic and psychological observation. On the one hand, the theory of transmission has induced investigators to trace the distribution and history of customs and beliefs with care so as to ascertain empirically whether they are spontaneous creations or whether they are borrowed and adapted. On the other hand, the psychic conditions that accompany various types of culture have received more careful attention.

These detailed archæological and ethnological studies have retroacted upon the theories of anthropology. The grand system of the evolution of culture, that is valid for all humanity, is losing much of its plausibility. In place of a simple line of evolution there appear a multiplicity of converging and diverging lines which it is difficult to bring under one system. Instead of uniformity the striking feature seems to be diversity. On the other hand, certain general psychic facts seem to become discernible, which promise to connect folk psychology with individual psychology. The trend of this development is familiar to us in the history of other sciences, such as geology and biology. The brilliant theories in which the whole range of problems of a science appears simple and easily explorable have always preceded the periods of steady empirical work which make necessary a complete revision of the original theories and lead through a period of uncertainty to a more strictly inductive attack of the ultimate problems. So it is with anthropology. Later than the older sciences, it has outgrown the systematizing period and is just now entering upon the empirical revision of its theories.

Our sketch of the history of the prevailing tendencies in anthropology would be incomplete without a few remarks on the men who have made it what it is. What has been said before shows clearly that there is hardly a science that is as varied in its methods as anthropology. Its problems have been approached by biologists, linguists, geographers, psychologists, historians and philosophers. Up to ten years ago we had no trained anthropologists, but students drifted into anthropological research from all the sciences that I have mentioned here and perhaps from others. With many it was the interest aroused by a special problem, not theoretical considerations, that decided their course. Others were attracted by a general interest in the evolution of mankind. The best among them were gradually permeated by the fundamental spirit of anthropological research, which consists in the appreciation of the necessity of studying all forms of human culture, because the variety of its forms alone can throw light upon the history of its development, past and future, and which deigns even the poorest tribe, the degraded criminal and the physical degenerate worthy of attentive study because the expressions of his mental life, no less than his physical appearance, may throw light upon the history of mankind.

Even now the multifarious origin of anthropology is reflected in the multiplicity of its methods. The historian or the political economist who comes into contact with

anthropological problems can not follow the methods of the biologist and of the linguist. Neither can the anthropologist of our period fill the demands for information of all those who may need anthropological data. It might almost seem that the versatility required of him will set a limit to his usefulness as a thorough scientist. However, the solution of this difficulty is not far off. We have seen that a great portion of the domain of anthropology has developed through the application of the new historical point of view to the mental sciences. To those who occupy themselves with this group of problems anthropological knowledge will be indispensable. Though the anthropological point of view may thus pervade the treatment of an older branch of science and help to develop new standpoints, the assistance that anthropology renders it does not destroy the independence of the older science which in a long history has developed its own aims and methods. Conscious of the invigorating influence of our point of view and of the grandeur of a single all-compassing science of man, enthusiastic anthropologists may proclaim the mastery of anthropology over older sciences that have achieved where we are still struggling with methods, that have built up noble structures where chaos reigns with us; the trend of development points in another direction, in the continuance of each science by itself, assisted where may be by anthropological methods. The practical demands of anthropology also demand a definition and restriction of its field of work rather than constant expansion.

The historical development of the work of anthropologists seems to single out clearly a domain of knowledge that heretofore has not been treated by any other science. It is the biological history of mankind in all its varieties; linguistics applied to people without written languages; the ethnology of people without historic records, and prehistoric archæology. It is true that these limits are constantly being overstepped, but the unbiased observer will recognize that in all other fields special knowledge is required which can not be supplied by general anthropology. The *general* problem of the evolution of mankind is being taken up now by the investigator of primitive tribes, now by the student of the history of civilization. We may still recognize in it the ultimate aim of anthropology in the wider sense of the term, but we must understand that it will be reached by cooperation between all the mental sciences and the efforts of the anthropologist.

The field of research that has been left for anthropology in the narrower sense of the term is, even as it is, almost too wide, and there are indications of its breaking up. The biological, linguistic and ethnologic-archæological methods are so distinct that on the whole the same man will not be equally proficient in all of them. The time is rapidly drawing near when the biological branch of anthropology will be finally separated from the rest and become a part of biology. This seems necessary, since all the problems relating to the effect of geographical and social environment and those relating to heredity are primarily of a biological character. Problems may be set by the general anthropologist. They will be solved by the biologist. Almost equally cogent are the reasons that urge on to a separation of the purely linguistic work from the ethnological work. I think the time is not far distant when anthropology pure and simple will deal with the customs and beliefs of the less civilized people only, and when linguistics and biology will continue and develop the work that we are doing now because no one else cares for it. Nevertheless, we must always demand that the anthropologist who carries on field research must be familiar with the principles

of these three methods, since all of them are needed for the investigation of his problems. No less must we demand that he has a firm grasp of the general results of the anthropological method as applied by various sciences. It alone will give his work that historic perspective which constitutes its higher scientific value.

A last word as to the value that the anthropological method is assuming in the general system of our culture and education. I do not wish to refer to its practical value to those who have to deal with foreign races or with national questions. Of greater educational importance is its power to make us understand the roots from which our civilization has sprung, that it impresses us with the relative value of all forms of culture, and thus serves as a check to an exaggerated valuation of the standpoint of our own period, which we are only too liable to consider the ultimate goal of human evolution, thus depriving ourselves of the benefits to be gained from the teachings of other cultures and hindering an objective criticism of our own work.

Franz Boas.

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*PLANT MORPHOLOGY.**

Those who organized these congresses left to the guests whom they honored with their invitation a high degree of freedom in the handling of their subject. In the exercise of that freedom, which I gratefully acknowledge, I have decided not to attempt any general dissertation on the present position of plant morphology as a whole, but to discuss certain topics only in the morphology of plants, which at present take a prominent place in that branch of the science of botany. These center round the question of the relation of the axis to the leaf in vascular plants.

* Address delivered at the International Congress of Arts and Science, St. Louis, September, 1904. The full text will be published in the official proceedings.

We may, I think, date the foundation of a scientific comparative morphology of plants from the publication of the 'Vergleichende Untersuchungen' of Hofmeister, and his recognition of the fundamental homologies between mosses, ferns and other plants. But notwithstanding the soundness of Hofmeister's comparisons for the alternating generations as a whole, the homologies of the parts remained unsatisfactory; the chief reason for this was that their grouping was not derived from the comparison of nearly allied species; nor does it seem to have been held as important to consider critically whether such parts as were grouped together were or were not comparable as regards their descent. For long years after the publication of the 'Origin of Species' homology had no evolutionary significance in the practise of plant morphology. But in the sister science of zoology this matter was taken up by Ray Lankester, in 1870, in his paper 'On the Use of the Term Homology in Modern Zoology, and the Distinction Between Homogenetic and Homoplastic Agreements.' (Many botanists of the present day would be the better for a careful study of that essay.) He pointed out that the term homology, as then used by zoologists, belonged to the Platonic school, and involved reference to an ideal type. This meaning lay at the back of Goethe's theory of metamorphosis in plants, and it seems to have been somewhat in the same sense that homologies were traced by Hofmeister. Lankester showed that the zoologist's use of the term 'homologous' included various things; he suggested the introduction of a new word to define strict homology by descent; structures which are genetically related in so far as they have a single representative in a common ancestor, he styled 'homogeneous'; those which correspond in form, but are not genetically related, he termed 'homoplastic.'

It is important at once to recognize that the strict 'homogeny' defined by Lankester as that of 'structures which are genetically related in so far as they have a single representative in a common ancestor' can only be traced in the simpler cases of plant form: it implies the repetition of individual parts, so strictly comparable in number and position as to stamp the *individual identity* of those parts in the successive generations. The right hand of a child repeats in position and qualities the right hand of the mother and of the race at large; here is a strict homogeny. In the plant body such individual identity of parts of successive generations is not common. It may be traced, for instance, in the cotyledons, and the first plumular leaves of seedlings of nearly related species, or in their first roots. But as a consequence of that continued embryology, which is so constant a feature in the plant body, the number of the appendages of any individual is liable to be indefinitely increased, while often the absence of strict rule in their relative positions makes their identical comparison in different individuals impossible. This is especially clear in the case of roots of the second, and higher orders; for they do not correspond in exact number or position in seedlings. What we recognize in such cases is then, not the individual identity, but their similarity in other respects; and when we group them under the same head we recognize, not their strict homogeny according to the definition of Lankester, but their essential correspondence, as based upon the similarity of their structure, and of their mode of origin upon, and attachment to, the part which bears them. This is also the case with the antheridia and archegonia of the pteridophytes, which are as a rule definite neither in number nor in arrangement, and are subject to variation in both respects, according to the conditions which may be imposed upon them by experiment; nevertheless they accurately maintain their structural characters, and their essential correspondence is thus established, but not their individual identity. It is clear that this is a comparison of a more lax order than the recognition of their individual homogeny would be.

But if room for doubt of the strictest homogeny be found in simple cases such as these, what are we to expect from the comparisons of less strictly similar parts of the plant, such as cotyledons, scale leaves, foliage leaves, bracts, sepals, petals, stamens, carpels? How far are these to be held to be homogeneous, or in some less strict sense homologous? Or, going still further, how are we to regard those comparisons which deal with parts of different individuals, species, genera, orders or classes? What degree of homology is to be accorded to them? In proportion as the systematic remoteness of the plants compared increases, and the continuity of the connecting forms is less complete, so the comparisons become more and more doubtful, and the use of the term 'homology' as applied to them more and more lax, until we are finally landed in the region where comparison is little better than surmise. It becomes ultimately a question how far the term 'homology' is to be held as covering these more lax comparisons, which are certainly not examples of 'homogeny' in Lankester's sense, and are only doubtfully correlated together on a basis of comparison of more or less allied forms.

The progress of our science should be leading towards a refinement of the use of the term 'homology'; an approach must be made, however distant it may yet be, to a classification of parts on a basis of descent. But though that may be readily accepted in theory, it is still far from being adopted in the general practise of

plant morphology. None the less, comparison is inevitably leading to the disintegration, on a basis of descent, of the old-accepted categories of parts; of these the most prominent, and at the same time the most debatable, is the category of leaves, and they will lend themselves best to the illustration of the matter in hand.

To those who, like myself, hold the view that the two alternating generations of the Archegoniatæ have had a distinct phylogenetic history, it will be clear that their parts can not be truly comparable by descent. The leaf of the vascular plant, accordingly, will not be the correlative of the leaf of a moss. Even those who regard the sporophyte as an unsexed gametophyte will still have to show, on a basis of comparison and development, that the leaves of the two generations are of common descent. I am not aware that this has yet been done by them.

But the phylogenetic distinctness of the leaves in the sporophyte and gametophyte is not the only example of parallel foliar development. Goebel has shown with much cogency that the foliar appendages of the bryophytes are not all comparable as regards their origin; he remarks, 'It is characteristic that the leaf formation in the liverworts has arisen independently in quite a number of series' ('Organographie,' p. 261), and has shown that they must have been produced in different ways. Here then is polyphyleticism in high degree, seen in the origin of those parts of the gametophyte which on grounds of descent we have already separated from the foliar appendages of the sporophyte.

Such results as these for the gametophyte lead us to enquire into the views current as to the origin of foliar differentiation in vascular plants. In discussing such questions, it is to be remembered that in different stocks the foliar condition of the sporophyte as we see it may have been achieved in different ways, just as investigators have found reason to believe that it was in the gametophyte. We have no right to assume that the leaf was formed once for all in the descent of the sporophyte. But at the moment we are unprovided with any definite proof how it occurred. All the evidence on the point is necessarily indirect, since no intermediate types are known between foliar and non-foliar sporophytes. Physiological experiment has as yet nothing to say on the subject. The fossil history of the origin of the foliar state in the neutral generation is lost, for the foliar character antedated the earliest known fossil-sporophytes. There remain the facts of development of the individual, and comparison, while anatomical detail may have some bearing also on the question; but all of these, as indirect lines of evidence, fall short of demonstration, and accordingly it is impossible to come at present to any decision on the point. For the purposes of this discussion, however, we shall proceed on the supposition that all leaves of the sporophyte generation originated in essentially the same way, though not necessarily along the same phyletic line.

There are at least three alternatives possible for the origin of the foliar differentiation of the shoot, in any progressive line of evolution of vascular sporophytes: (1) That the prototype of the leaf was of prior existence, the axis being a part which gradually asserted itself as a basis for the insertion of those appendages; the leaf in such a case would be from the first the predominant part in the construction of the shoot. (2) That the axis and leaf are the result of differentiation of an indifferent branch-system, of which the limbs were originally all alike; in this case neither leaf nor axis would predominate from the first. (3) That the axis preexisted, and the foliar appendages arose as outgrowths upon it; in

this case the axis would be from the first the predominant part.

The first of the above alternatives, viz., that the prototype of the leaf existed from the first, and was indeed the predominant part in the initial composition of the shoot, has been held by certain writers as the basis of origin of the leafy shoot in vascular plants.* On this view not only is the whole shoot regarded as being mainly composed of leaves, but some even contend that the axis has no real existence as a part distinct from the leaf bases.†

This view in its general form represented the plant as constructed on a plan somewhat similar to that of a complex zoophyte. It has more recently culminated in the writings of Celakovsky and Delpino. The former in his theory of shoot-segments ('Sprossgliedlehre') starts from the position that the plant is composed of morphological individuals; the cell, the shoot and the plant stock are recognized as such. The stock is composed of shoots, and the shoot of cells. Braun recognized the shoot as the individual *par excellence;* between the cell and the shoot is a great gulf, which has not yet been filled; 'between the cell and the bud (shoot) there must be intermediate steps, the limitation of which no one has succeeded in defining'; the long sought for individual middle step is the shoot segment (Spross-glied), which is neither leaf only, nor stem segment only, but the leaf together with its stem segment. Now this appears to me to be purely Platonic morphology; the intermediate step *must* occur; we will, therefore, discover and define it. The definition of it consists in the drawing of certain transverse and longitudinal lines partitioning the shoot, lines which in the sporophyte have no existence in nature; the assumed necessity of partitioning the shoot into parts of an intermediate category between the whole shoot and the cell brings these assumed limits into existence.

Notwithstanding the ingenuity of the theory as put forward by Celakovsky, in the absence of any structural indication of the limits of the shoot segments in the vast majority of cases the theory does not appear to me to be sufficiently upheld by the facts.

An extreme, and indeed a paradoxical position has been taken on this phytonic question by Delpino. As a consequence of his studies on phyllotaxis he concluded that the axis is simply composed of the fusion of the leaf bases; that the leaves are not appendicular organs, but central organs; that an axis or stem system does not exist, and accordingly that the higher plants are not cormophytes at all, but phyllophytes. There will, I think, be few who will adopt this fantastic view of the shoot.

The second view, that the axis and leaf are the result of differentiation of an indifferent branch system, of which the limbs were originally all alike, has lately been brought into prominence by Potonié.* Taking his initiative from the branching of the leaves in early fossil ferns, he recognizes the frequent occurrence of overtopping ('Uebergipfelung'), that is, the gradual process of assertion of certain limbs of a branch system over others; in the branching of fucoids he finds an analogy for his observations on fern leaves, and draws the following conclusion, that 'the leaves of the higher plants have been derived in the

* Goethe, 'Die Metamorphose der Pflanzen.' Gaudichaud, *Mem. de l'Acad. d. Sci.*, 1841. Kienitz Gerloff, *Bot. Zeit.*, 1875, p. 55. Celakovsky, 'Unters. ueber die Homologien,' *Pringsh. Jahrb.*, XIV., p. 321, 1884; *Bot. Zeit*, 1901, Heft. v., VI.

† Delpino, 'Teoria generale della Filotassi.' For ref. see *Bot. Jahresbr.*, VIII., 1880, p. 118, also Vol. XI., 1883, p. 550.

* 'Lehrbuch d. Pflanzenpalaeontologie,' pp. 156–159. Also 'Ein Blick in die Geschichte d. Bot. Morph. und d. Pericaulomtheorie,' 1903, p. 33, etc.

course of generations from parts of an algal thallus like that of *Fucus,* or at least from alga-like plants, by means of the overtopping of dichotomous branches, 'and the development as leaves of the branches, which thus become lateral.' Dr. Hallier, who adopts Potonié's position, prefers to draw the comparison with liverworts, which show a similar sympodial development of a dichotomous branch system.*

It seems not improbable that the condition of many branched fern leaves may have been derived through a process of 'overtopping' in an indifferent branch system of the leaf itself. But it lies with Potonié to show, on a basis of comparison of forms more nearly related to them than the fucoids, that the relation of axis to leaf in the ferns was so derived; and, further, that such an origin is in any way applicable to other foliar developments in vascular plants, especially pteridophytes such as the lycopods, equiseta and sphenophylls. I am not aware that this has yet been done. But granting that this can be done, the question still remains whether similarity of method of branching is any criterion of comparison as to descent. And especially whether such comparison is valid between widely distant groups, or between the different generations of an antithetic alternation. It is true that Potonié prefers to regard such generations as homologues, as is indeed essential for his view; but that does not prevent others from differing from him, or even considering the fact that the parts compared belong to different generations as fatal to his theory. For my own part, I am not prepared to give up the broad conclusions as to antithetic alternation on so slender a ground as similarity of method of branching represented in them both. For sympodial development of a dichotomous system (and this is all that such 'overtopping' actually is) has occurred in cases where it can not be held to have resulted in a branching which is foliar; and of this instances can be found without going so far afield as the Fucaceæ. If this be so, then little value need be attached to the comparison of such branchings in plants not nearly allied to one another; these may be held to be quite distinct examples of a general phenomenon, without the one being in any sense the prototype of the other. Such reflections as these indicate that the comparison in mode of branching between the leaves of ferns and the thallus of fucoids, which forms the groundwork of the view of Potonié (or between the ferns and the thalloid liverworts, as may be preferred by others), are not to be held as more than distant analogies; consequently they are no demonstration of the origin of the leaf by a process of 'overtopping.'

There remains the third view, which, however, is no new one; for there have not been wanting those who have assigned a more prominent place to the axis in the initial differentiation of the shoot. Perhaps the most explicit statement on this point is that by Alexander Braun, who remarks in his 'Rejuvenescence in Nature' (English edition, p. 107), referring to phytonic theories, that 'all these attempts to compose the plant of leaves are wrecked upon the fact of the existence of the stem as an original, independent and connected structure, the more or less distinct articulation of which certainly depends upon the leaf formation, but the first formation of which precedes that of the leaves.' Unger also in his botanical letters to a friend (No. VIII.), described how 'The first endeavor is directed towards the building up with cell-elements of an axis'—'those variously formed supplementary organs which are termed leaves originate laterally upon it'

* 'Beitrage z. Morph. d. Sporophylle u. d. Trophophylls,' Hamburg, 1902.

and he concludes that 'we may [therefore] say with perfect justice that the plant * * * is, as regards form, essentially a system of axes.' Naegeli contemplated a somewhat similar origin of the leafy shoot, as an alternative possibility; in fact, that the apex of a sporogonium-like body elongated directly into that of the leafy stem, in which case the axis would be the persistent and prominent part, and the leaves be from the first subsidiary, and lateral appendages. In my theory of the strobilus in archegoniate plants the central idea was somewhat similar to this. It may be briefly stated thus: There seems good reason to hold that a body of radial construction, having distinction of apex and base, and localized apical growth as its leading characters, existed prior to the development of lateral appendages in the sporophyte; for such a body is seen in certain bryophyte sporogonia, while the prior existence of the axis and lateral origin of the appendages upon it are general for normal leafy shoots. The view thus put forward is, indeed, the mere reading of the story of the evolution of leaves in terms of their normal individual development. I have recently shown that all pteridophyte shoots may be regarded as derivatives from the radial strobiloid type, with relatively small leaves, which would thus have come into existence.

It is natural to look to the pteridophytes for guidance as to the origin of foliar development in the sporophyte, for they are the most primitive plants with leafy sporophytes. They may be disposed according to the prevalent size of their leaves in a series, leading from microphyllous to megaphyllous types. I have lately shown that such a seriation is not according to one feature only, but that certain other characters which have been summarized as 'filicineous' tend to follow with the increasing prominence of the leaf. This indicates that such seriation is a natural arrangement. Now it is possible to hold either that the large-leaved fern-like plants were the more primitive, and the smaller-leaved, derivatives from them by reduction; or, conversely, that the smaller-leaved were the more primitive, and the larger-leaved derivatives from them by leaf-enlargement; other alternative opinions are also possible, such as that the leaf origin has been divergent from some middle type, or that the leaves of vascular plants may have been of polyphyletic origin.* For the moment we shall leave these latter alternatives aside.

Much of the difference of view as to foliar origin centers round the question whether originally the leaf was relatively large or small. Those who hold that the large-leaved forms were the more primitive will be naturally disposed towards the view of the original preponderance of the leaf

* The view recently advanced by Professor Lignier ('Equisetales, et Sphenophyllales. Leur origine filicinienne commune,' *Bull. Soc. Linn. de Normandie*, Serie 5, Vol. 7, Caen, 1903) is analogous to that of Potonié, though differing from it in detail. It involves the ranking of the lycopod leaf as a 'phylloid,' the leaf of the fern as a true leaf, or 'phyllome,' differentiated from an indifferent system of 'cauloids,' on which the 'phylloid' appendages had become abortive. It regards the leaves of equiseta and sphenophylls as phyllomes, reduced from the larger-leaved fern-type. The argument is chiefly based on comparisons as to branching and anatomical structure. I do not think that these grounds suffice to override the probability that the leaves of lycopods are essentially of the same nature as those of the sphenophylls or equiseta (compare my 'Studies,' No. V.). Professor Lignier's view further involves the acceptance of homologous alternation, while he makes no mention of the chromosome-differences of the two generations. Such difficulties do not arise if the leaves of the sphenophylls and equiseta are regarded as being in the upward rather than the downward scale of development, a view of them which would equally harmonize with the anatomical comparisons of Professor Lignier.

over the axis, and will favor some phytonic theory; those who hold the smaller-leaved forms to be the more primitive will probably adopt a strobiloid theory of origin of the leafy sporophyte. I propose to offer some remarks on the relative probability of these alternative views.

If large-leaved prototypes be assumed generally for vascular plants, this naturally involves a widespread reduction, since small-leaved forms are numerous now, and have been from the earliest times of which we have any record. Reduction is a ready weapon in the hands of the speculative morphologist, and it has often been used with more freedom than discretion. But reduction should never be assumed in order to meet the demands of convenience of comparison, nor as a cover for doubt. The justification of a view involving reduction must be found in its physiological probability in the case in question, and this should be backed by comparisons of form and of anatomical structure; the conclusion should also be in accordance with the paleontological record. All suggested cases of reduction where such justification is absent should be looked upon as doubtful.

Convincing evidence of reduction of leaf complexity in an evolutionary sequence, supported on all these grounds, has been adduced in the progression from ferns, through cycado-filicinean forms, to the cycads; and it applies with special force in the case of their sporophylls. Ferns, which are essentially shade-loving and typically zoidiogamic, or amphibious, may be understood to have given rise to the cycado-filices, and cycads, which are more xerophytic, and show that essential character of land plants—the seed habit. Not only is such a progression physiologically probable, but it is supported by paleontological evidence, as well as by detailed facts of anatomy, and of reproductive morphology. The case for reduction of leaf complexity seems to be here fully made out, and somewhat similar arguments will also apply for other types of gymnosperms.

The facts relating to the vascular system of the shoot have also their bearing on the question of the relative size of primitive leaves. The origin of the leaf trace from the axial stele in conifers, and also in angiosperms, has been shown by Dr. Jeffrey to be of the type styled by him phyllosiphonic. This is specially characteristic of those plants where the leaf is essentially the dominating influence in the shoot. In this I see a probability, which their physiological position as land-growing plants would justify, that the seed-bearing plants at large were descended from a large-leaved ancestry, and had undergone reduction of leaf complexity in their descent. But while we thus recognize a probability of widespread reduction producing relatively smaller-leaved forms, it does not follow that *all* small-leaved vascular plants originated thus. On this point the anatomical evidence is of importance, as bearing on the origin of the small-leaved strobiloid pteridophytes. Of these (putting aside the hydropterids as being a special reduction problem in themselves), there remain the Lycopodiales, the Equisetales and the Sphenophyllales, which are all cladosiphonic in the terminology of Dr. Jeffrey. The question will largely turn upon the meaning of this anatomical feature. I take it to be as follows: The cladosiphonic character is the anatomical expression of the dominance of the axis in the shoot. Here the leaf trace is merely an external appendage on the stele, which is hardly disturbed by its insertion. This type is seen in certain small-leaved pteridophytes. The phyllosiphonic character, on the other hand, is the anatomical expression of the dominance of the leaf over the axis in the shoot. Here the insertion of the vascular supply of the leaf profoundly disturbs the

vascular arrangement in the axis. It is characteristic of certain large-leaved pteridophytes, and is seen also generally in seed plants.

It is a fact of importance that, in the individual life, the one or the other type is usually constant; but in certain ferns the progression may be traced from the cladosiphonic in the young plant to the phyllosiphonic in the mature, thus suggesting a similar progression in descent, viz., that the large-leaved phyllosiphonic ferns were derived from a smaller-leaved cladosiphonic stock. Of the converse, viz., the progression from the phyllosiphonic to the cladosiphonic state in the individual life, I know of no example among the pteridophytes, though it is true that there is some approach to it in the Marsileaceæ. Thus the anatomical evidence indicates a probability that, even in large-leaved ferns, the cladosiphonic was the primitive type; but that the phyllosiphonic, once initiated, is as a rule maintained; this is shown by its persistence in the seed plants, even where the leaf has been reduced in size.

Having thus gained a valuable sidelight from anatomy, we may now return to our central question, of the initial relation of leaf to axis. Of the three theories already noted, the theory of overtopping as applied to the origin of the leaf, may in my opinion be dismissed, as it is not based upon comparison of nearly related forms, while the sympodial development of a dichotomous system, on which it is founded, is a general phenomenon of branching, restricted neither to leaves nor to the sporophyte generation. As to the other two, the facts whether of external form or of internal structure seem to me to indicate this conclusion: that the strobiloid condition was primitive for certain types, such as the Equisetales, Lycopodiales and Sphenophyllales; that in them the leaf was from the first a minor appendage upon the dominating axis; and anatomically they have never broken away from the cladosiphonic structure, which is the internal expression of their microphyllous, strobiloid state. That the Filicales and also the Ophioglossales were probably derived from a microphyllous strobiloid ancestry, and achieved the phyllosiphonic structure as a consequence of leaf enlargment, this being the derivative rather than the primitive condition; its derivation is even illustrated in the individual life of some ferns. From the Filicales the phyllosiphonic structure was probably handed on to the seed plants, and by them retained, notwithstanding the subsequent leaf reduction which followed on their adaptation to an exposed land habitat. Thus a strobiloid origin may be attributed to all the main types of vascular plants. It seems to me to harmonize more readily with the facts than any phytonic theory does.

A prototype, which was probably a prevalent, though perhaps not a general, one for the pteridophytes, may then be sketched as an upright, radial, strobiloid structure, consisting of a predominant axis, bearing relatively small and simple appendages. On our theory the origin of those appendages in descent would be the same as it is to-day in the individual development, viz., by the outgrowth of regions of the superficial tissue of the axis to form them. The axis would preexist in descent, as it actually does in the normal developing shoot. The origin of these appendages may have occurred independently along divers lines of descent, and the appendages would in that case be not homogeneous in the strict sense. Thus there would be no common prototype of the leaf, no morphological abstraction, or archetypic form of that part. More than one category of appendages might even be produced on the same individual shoot, differing in their function on their first appearance. Such has perhaps been

the case in the calamarian strobilus, where the leaf tooth can not be readily homologized with the sporangiophore. These suggestions will suffice to indicate how elastic a strobiloid theory is, and how its application will cover various types of construction, even such as are shown by the most complex cones of pteridophytes.

From the comparison of living species there is good reason for thinking that all the primitive leaves in certain types, such as the lycopods, were sporophylls, and that a subsequent differentiation took place, by abortion of the sporangia; thus a sterile vegetative region became defined from a fertile upper region. It may be a question whether this origin by sterilization of sporophylls is applicable to foliage leaves at large. Nevertheless, analogy, not only with other vascular plants but also with the bryophytes, suggests that a similar differentiation of a sterile from a fertile region has been a general phenomenon in the neutral generation. At first in the simpler pteridophytes these regions were essentially similar to one another in form, as is still seen to be the case in some lycopods. Later, however, the sterile and fertile regions took divergent lines of development in accordance with their difference of function. The differentiation reaches its climax in the higher flowering plants. The inflorescence, or flower, on this view, though produced later than the vegetative region in the individual life, embodies the more primitive parts, viz., those which bear the sporangia and spores. The vegetative region is in its origin mostly, if not wholly, secondary. The physiological reasonableness of this view is too obvious to need insistence. As the self-nutritive powers of the gametophyte fell off in the adaptation to the land habit, the nutritive function was taken up by the new vegetative system thus intercalated between sexual fusion and spore production.

This is in brief outline the strobiloid theory of the shoot in vascular plants, as arising out of the facts of antithetic alternation. It will be seen that it is essentially in harmony with the view of Braun, upheld also by Sachs, that the shoot is the real morphological unit, of which leaf and axis are correlative parts. Those who adopt it will find their position simplified in regard to another question which has recently taken afresh a prominent place in morphological discussions, viz., the theory of cortication (Berindungstheorie). It is held by Potonié, and a similar view was also maintained by Celakovsky, that the stem has centrally an axial nature, peripherally a leaf nature. The primitive axis (Urcaulom) acquires in the course of generations, by coalescence with the basal parts of its primitive leafy appendages (Urblätter), a mantle,—a 'Pericaulom.' This is what we commonly designate the cortex, which is thus regarded as not being axile in origin, but foliar. In accordance, however, with our strobiloid theory we may presume that, as is seen in some of the bryophytes, the simple sporophyte consisted originally of a central region—a primitive stele—and a peripheral region, a primitive cortex. From the latter sprang the appendages, as superficial outgrowths, just as at the present day the leaves originate upon the cortex of the axis. The cortex in such cases would be, from the first, part of the primitive axis, and the outgrowths processes from it. The primitive cortex from which the appendages sprang may remain a continuous, undifferentiated band, as it actually does appear in the vast majority of leafy sporophytes; or it may be in certain cases more or less clearly marked off into regions surrounding the insertion of the individual leaves. But in the fact that these special cases exist I see no sufficient foundation for the view that each leaf is, in shoots at

large, connected with a definite area of extended leaf-base; and still less for the theory that in vascular plants the cortex originated from such coalescent leaf bases. Our theory of the strobilus would, indeed, presuppose that close relation of cortex and appendage, and absence of limit between them, which is so common a feature in vascular plants; and furthermore, it will readily cover the facts where the cortex is delimited into definite areas round the leaf bases; but it does not recognize any necessity for generalizing from such cases of special delimitation that the cortex is foliar in its origin, in shoots of vascular plants at large. It would be more ready to suggest the converse, viz., that the leaves were cortical in their origin, as, indeed, they are in the ontogeny.

Discussions such as these on phytonic theory, or theory of cortication, are liable to develop into mere scholastic contests. They originated in the present case in the use of terms in an unprecise sense, and the subsequent attempt to attain precision. Both these theories have proceeded from the assumption that the 'leaf' is an abstract entity, distinct from the stem. Difficulties arise when the attempt is made to carry out that distinction sharply in practise, for this is nothing less than the attempt to define precisely things which in point of fact appear neither uniform nor precise in nature. The strict definition of terms used in morphological science is doubtless in itself a desirable thing; but it must be so conducted as to harmonize with the facts of individual development, while at the same time it must not violate evolutionary probability. As a matter of fact, neither in the mature state nor in the ontogenetic or phylogenetic development of the leaf does the structure suggest its sharp delimitation from the axis as a general feature in the shoots of ordinary vascular plants.

My present position with regard to the phytonic theories and the theory of cortication is frankly destructive; for in the first place, if the evidence from the gametophyte generation be discounted, the facts of segmentation in the sporophyte are of the slenderest; further, I do not think that morphological insight will be advanced by attempts to define precisely the limits of the parts of the vascular shoot; it seems more in accordance with nature to accept for vascular plants the view of Braun and of Sachs, that the shoot is the original unit. What is first urgently required, in order to decide such questions, is the correct recognition of the phyletic lines which eventuated in the various appendages as we see them. Then may follow definitions of the parts, which may or may not succeed in assigning their strict limits. When this is accomplished a terminology may follow which shall segregate parts which have had a separate phyletic origin. Thus an evolutionary morphology of the shoot would be built up. But it is useless to accept the thesis merely in the abstract that the basis of morphology must be in phylogeny; the principle must also be put in practise, and be ultimately reflected in our methods, and in the definition of our terms.

A step in this direction will be the recognition that at present the word 'leaf' is loosely applied; it is, indeed, a temporary makeshift borrowed from colloquial language, and used in a descriptive rather than in a strictly scientific sense. It designates collectively objects which have, it is true, formal and functional, and even topographical, features in common, but have not had the same phyletic history. There is every probability that the word 'leaf' will continue to be used in this merely popular sense.

This position, with its conservative use of terms fitting awkwardly upon advanc-

ing phyletic ideas, can only be properly understood by glancing back at the history which has produced it. So long as species were regarded as the individual results of creative power, the complexity and variety of their form were relegated to the arcana of the divine mind, and organic nature presented the aspect of a series of isolated pictures; any similarity which these might show was to be regarded as indicative of the underlying divine plan. Now that species have been threaded together by evolutionary theory into developmental sequences, they, like the ribbon of a cinematograph, present phyletic history to the mind with all the vividness of a living drama. While monophyletic views held the field, this seemed comparatively simple; but the conclusions thus arrived at in plant morphology were often palpably improbable. Such difficulties, together with the substantiation of examples of parallel development on a sound comparative basis, led to the modification of monophyletic views, and opened the way for less cramped conceptions. It is now customary to contemplate the plural origin of such leading features as sexual differentiation, foliar development, heterospory, the seed habit, as well as a host of minor characters. On such examples we base a general belief that similar structures may be arrived at by divers evolutionary routes. It is this conception of polyphyleticism that we must make clear in our descriptions, if not even in our terminology.

It will be objected that to carry through a method of designating by the same term only such parts as are shown to be of common descent would produce unwieldy results. Doubtless this is true, but in the terminology of a science it is not so much convenience, as truth and clearness which should be the aim. The choice is open to us either to make the terminology strictly phyletic throughout, which would certainly be cumbrous, though it would reflect the true position, or, putting phyletic distinctions in the background, to use terms in a more or less comprehensive sense, even grouping together things which we know to have been distinct in phyletic origin. Such a comprehensive sense is conveyed by the expression 'homology of organization,' which, as Goebel points out, 'has only to do with phylogeny in so far as it recognizes a common capacity for development derivable from undifferentiated ancestors.'*

This is, indeed, a collective term for the results of parallel development; it suffers from the danger of suggesting some ideal type, or pattern, towards which evolution has tended.

For my own part, I think it matters little what our terminology be, or what the separation of categories of parts, provided we attach clear meanings to the words we use, and select those words as naturally conveying that meaning. For instance, if we fully realize that the word 'leaf' is used in a sense which is not phylogenetic, but merely descriptive of those lateral appendages on the shoot which are produced exogenously, and in acropetal order, then let it remain, ranking as an expression of 'homology of organization.' But the appendages thus included may for clearness be conveniently divided into 'phyllomes' on the sporophyte and 'phylloids' on the gametophyte, as indeed I suggested some years ago. Nevertheless, these again are not phyletic unities; they include parts with distinct histories which have already been recognized in the gametophyte, while for the sporophyte a more advanced state of the science will probably provide definitions. Meanwhile we consent to a compromise in grouping these together; but the only condition upon which this can be safely done is the clear knowledge that this is a compromise by which we secure a cer-

* 'Organographie,' English edition, p. 19.

tain convenience of description. Moreover, the acceptance of this compromise must not be understood to grant free license to argue from one to another of the forms included, as though they were equivalents; what has resulted in one line of descent can at best only throw a side light on what has happened in another distinct line, and in proportion as the lines involved in a comparison are more remote from one another, their comparison assumes more and more the character of a mere analogy. The danger which our compromise brings with it is that this will not be clearly kept in mind. At all hazards the strict phyletic view should underlie all present morphological discussion, notwithstanding that, for mere convenience, that view may not be clearly reflected in the classification of the parts. This makes me hope that the compromise is only a temporary concession, and that it will give way ultimately to the demands which a more detailed knowledge of descent is sure to bring.

It is well, however, in connection with discussions such as these, to impress upon the lay public that all evolutionary theories are, like other scientific theories, hypotheses incapable of complete proof. No one will appreciate this more fully than biological investigators themselves, for they are in the best position to know how insufficient the evidence actually is, and how liberal a use has to be made of the imagination in bridging over the wide gaps in the series of known forms. The details of a story thus constructed depend so largely on comparative opinion, and in so slight a degree on positive demonstration, that the history as told by competent experts in comparative morphology may vary in material features. A little more weight allowed for certain observed details, or a little less for others, will be sufficient to disturb the balance of the evidence derived from a wide area of fact, and consequently to distort the historical picture. There is in truth no finality in discussions on the genesis and progress of organic life, or in the kaleidoscopic changes of opinion, since any new fact of importance will in some degree affect the weight accorded to others, and may vary the general result. It will be objected that conclusions which are so plastic are little better than expressions of personal taste; that the study of comparative morphology, is, therefore, calculated to dishearten its votaries, while the non-specialist public, which is compelled to take its information at second hand, will be bewildered, and will conclude that it is useless to pursue a subject which shows so little stability. But, on the other hand, those who follow the progress of morphology with sympathetic care will take heart when they compare its present position with that of a generation ago; it is encouraging to think that it is little more than half a century since the history of the life cycle of a fern was first completed. In some sixty years a vast array of kindred facts has been acquired, and a theoretic structure is being raised upon them which, though still protean, is gradually acquiring some settled form. Never has the advance of morphological thought been more rapid than at present. The support of the facts of alternation from the unexpected quarter of minute cytology has been one of the most striking features in the recent history of our science. The discovery of spermatozoids in the cycads and Gingkoaceæ has filled in a gap in the story of evolution which all followers of Hofmeister must have felt. But in no field of morphological research has investigation been more amply rewarded than in paleophytology. The luminous facts derived from fossils are shedding fresh light on obscure problems, such as the origin of the seed habit, and helping us to locate such difficult groups as the Psilotaceæ and

Equisetaceæ. When we regard these rapid advances, and truly estimate the influence they bring to bear upon morphological theory, we must surely congratulate ourselves on being devotees to a science which is very actively alive.

But at the same time the detached cynic may find in the methods of plant morphologists, or still more sometimes in their want of method, food for much critical remark. And if he put his finger upon one mental process which more than another has introduced discord, it would, I think, be 'assumption.' It may be that our science is not worse than others in this respect; but I am very sure that arguments based upon ill-founded assumption have put back the progress of morphology more than anything else in our discussions. Any one can find examples for himself in the literature; some of us in our own writings. It remains for us who tread the difficult path of morphological theory to beware lest we neglect those warnings with which its course is so plentifully strewn; for it is just as much the duty of a scientific man to avoid blurring the issues for others by faulty argument, as it is to attempt to make clear to them what he himself believes to have been obscure.

F. O. Bower.

University of Glasgow.

SCIENTIFIC BOOKS.

The Belgian Antarctic Expedition. Resultats du Voyage du S. Y. Belgica en 1897, 1898, 1899, Zoologie. Nématodes libres. Dr. J. G. de Man. April 15, 1904; 51 pp., 4to, pl. i–xi. *Bryozoa,* by A. W. Waters. February 15, 1904; 114 pp., 4to, pl. i–ix.

The report on the free nematodes considers four fresh-water forms truly Antarctic, and six marine species collected in the Magellanic region, of which, however, one had been originally described from South Georgia. Three of the Antarctic forms are new, one being supposed to belong to a new subgenus (*Plectoides*); the other form, a *Dorylaimus,* is too young for determination. The other three belong to the genera *Mononchus* and *Plectus.* This little group has a special interest in being the first known fluviatile forms from the Antarctic continent. All the species are treated at great length and profusely illustrated.

We learn from Water's report that 86 species of Antarctic bryozoa were collected; on one occasion 55 species were obtained at one haul of the tangles. Eleven others from the extralimital subantarctic waters are also considered.

Of the 86 species and varieties of Antarctic origin 57 are new, many of which are very closely related to already known northern forms.

Of the Chilostomata only seven are known from the northern hemisphere, all of which are also known in the fossil state. Three species are cosmopolitan and also Arctic. But little support is given to the 'bipolar' theory by the Bryozoa considered in this paper. The specimens of *Hornera lichenoides,* long since reported as brought from the Antarctic by Sir James Ross, there is much reason to believe did not come from that region, as they agree with Arctic and do not agree with Antarctic specimens of that genus. *Orthopora, Cellarinella* and *Systenopora* are described as new genera, all of which are Antarctic. A new species of *Alcyonidium* and seven new *Cyclostomata,* with seven others previously known, and one indeterminate, complete the enumeration.

W. H. Dall.

Smithsonian Institution.

SCIENTIFIC JOURNALS AND ARTICLES.

In the *Botanical Gazette,* for September, M. A. Chrysler has written upon 'The development of the central cylinder of Araceæ and Liliaceæ,' developing in these groups the recent stelar theories and reaching the general phylogenetic conclusion that monocotyledons are derived from dicotyledonous ancestors.—D. S. Johnson has given an account of 'The development and relationships of Monoclea,' a Jamaican liverwort.—W. C. Coker has writ-

ten a brief account of 'The Spores of Certain Coniferæ,' describing certain features of the male gametophyte and the megaspore.—G. J. Peirce has published a preliminary notice of 'Artificial parasitism,' giving an account of his experiments in growing a pea on a plant of horse bean, the pea blossoming and setting seed.—Albert C. Herre records the remarkable growth of the thallus of *Ramalina reticulata*.

PROFESSOR BURTON-OPITZ, of Columbia University, has undertaken the charge of the abstracts of American publications on the relations of medicine and chemistry for the *Biochemische Centralblatt*. Authors are requested to send him reprints.

SOCIETIES AND ACADEMIES.

CLEMSON COLLEGE SCIENCE CLUB.

THE first regular meeting of the club for the year was held on September 29, at 8 P. M., in the lecture room of the Electrical Laboratory of Clemson College. Professor W. M. Riggs discussed 'Recent Advances in Electrical Engineering.' Dr. R. N. Brackett presented a paper on 'The Present status of the Nitrogen Problem.'

HAVEN METCALF,
Secretary.

DISCUSSION AND CORRESPONDENCE.

VARIÆ AUCTORITATIS.

TO THE EDITOR OF SCIENCE: In looking over the numbers of SCIENCE on my return from my summer's work in the Rocky Mountains I find in your issue for August 12 a communication from Dr. C. R. Eastman entitled 'Variæ Auctoritatis,' in which he complains of the carelessness of scientific writers in citing ancient authorities, and brings me in at the head of his list of offenders quoted.

While agreeing with Professor Eastman in the general principle laid down by him, his remarks with reference to me seem to flavor of a certain disingenuousness.

He refers to my paper on 'The Theories of Ore-deposition Historically Considered,' in which, on page 2, I am made to say that 'It is said that as early as Origenes, 600 B. C., etc.,' whereas Origenes flourished about 200 A. D. Dr. Eastman very courteously pointed out this error in a personal letter to me in June last, and in reply I stated that this was something that had escaped my notice in proof-reading, and that the sentence should have read, 'Thus it is said by Origenes that as early as 600 B. C., etc., etc.,' at the same time referring him to my authority, Professor K. von Zittel, who, on page 3 of his 'History of Geology and Paleontology' says, 'Origenes reports of Xenophanes of Colophon that he had observed sea shells on mountains, etc., etc.'

With regard to his second criticism, I am unwilling to admit any error in my words as published. In opening my section on the 'Scientific Period' I remark that 'Up to this time (the close of the eighteenth century) even the name geology had hardly been recognized, natural history or mineralogy being the title usually given to works that treated of it.' Later, in speaking of De Saussure's work I say, 'He also appears to have been the first to use the name geology for *his* science.' Dr. Eastman says that my statement is incorrect, inasmuch as Richard de Bury spoke of Geologia as the 'earthly science' in 1344. That this learned prelate, or some other philosophical writer of an even earlier date, may have used the word does not disprove the correctness of my statement that De Saussure was the first (geologist) to use this term for *his* science. My statement does not necessarily imply that he was the first man who ever used the word, and I should have considered it a useless waste of time to have searched all ancient literature to find out by whom it was first used, for I do not feel at all sure that de Bury is entitled to that credit.

S. F. EMMONS.

THE SWALLOWING OF STONES BY SEALS.

TO THE EDITOR OF SCIENCE: So far as I am aware no satisfactory reason has been advanced for the swallowing of stones by seals, and this statement may probably be extended to other animals. They are not taken in for ballast, for the empty seals keep down as easily as the others; they are not swallowed for the purpose of grinding up food, for they occur in the stomachs of nursing pups; they are not

to allay the irritation caused by parasitic worms, since the two are by no means found together; they are not taken in with food, not merely because they are found in the stomachs of young seals, but in those adults that have fed on squid caught in the open sea. On page 68, Vol. III., 'Report of Fur Seal Investigation,' it says: "It is evident that these things are not swallowed haphazard, but are selected with considerable care from among the articles strewn along the shore, and that a preference is exhibited for rounded objects. This is shown by the fact that, as a rule, only articles of one kind are found in one stomach, although seals do not discriminate between fragments of barnacles and fragments of gastropods. Moreover, pebbles of serpentine and chalcedony are now and then found on the hauling grounds under conditions indicating that they were brought there by the seals, while the pup seen gathering pebbles on Lukanin did so with great care, by no means taking the first that came to hand. The most striking example of this discriminative selection is, perhaps, shown by the pup which had swallowed a buckshot, while the chance of finding such a thing must be, at a guess, about one in a million."

Furthermore, it may be said that as the fur seal regurgitates bones and other indigestible things, the supply of stones must be renewed from time to time.

That there is any connection between the presence of stones and the presence of a gizzard does not follow.

F. A. Lucas.
Brooklyn Institute Museum.

ANIMALS AT ROME IN ANCIENT TIMES.

Since the publication in Science (No. 506) of a short article entitled 'Ancient Natural History Lore,' the attention of the writer has been called to an interesting essay on a kindred subject by Countess Cesaresco. It is to be found in the August number (1904) of the *Contemporary Review*, under the title of 'Animals at Rome,' and presents a tolerable conception of the state of natural history amongst Romans of the Augustan age.

A work that was highly popular from the third century onward, and regarded as a standard authority on zoology, though comparatively little known at the present day is Ælian's treatise on the 'Nature of Animals.' The fifteenth book contains the earliest mention in history of the use of the artificial fly, and gives particulars of other modes of fishing in vogue during the olden time.

C. R. E.

SPECIAL ARTICLES.

ORNITHOLOGICAL NOTES FROM THE NEW YORK ZOOLOGICAL PARK.

A number of common terns (*Sterna hirundo*), least terns (*Sterna antillarum*), black skimmers (*Rhynchops nigra*) and laughing gulls (*Larus atricilla*) have recently been hatched from eggs in an incubator and reared in the New York Zoological Park. The eggs were all collected on the coast of Virginia.

Some interesting observations have been made for two seasons in connection with these birds, of which certain facts possess a special value and conclusiveness on account of the absolute isolation of these young sea-birds from adults of their species.

The sight of small but entire fish excites a newly-hatched black skimmer much more than does macerated fish, but the reverse is true of the terns until after the first week. Half-boiled and macerated fish does not quite take the place of what the parents of the terns would provide, but fish regurgitated by wood ibises after being held in the crop for two hours is a perfect substitute.

All the birds which were hatched in my experiments refused salt water, for both drinking and bathing, when fresh water was provided.

The remarkable disparity in the length of the upper and lower mandibles of the black skimmer is distinctly foreshadowed in the embryo of about the beginning of the third week.

The call, flock and alarm notes of all four species are wholly instinctive; neither taught by the parents nor learned by imitation. The first class of notes are uttered at the instant when the egg-tooth pierces the shell. The utterance which I have called the *flock* note differs decidedly from the call of the young for food, and is not uttered until the bird is

a day or two old. When several are together, this note seems to attract them to each other, while the ordinary call-note does not, and the alarm note has the very opposite effect.

There is absolutely no *instinctive* fear of man, cats, dogs, hawks or other objects, if these enter quietly into the environment of the birds, but a sudden shadow or unusual noise causes them to utter the characteristic cries of alarm (rarely), or usually to perform certain acts—wholly instinctive—which have for their object an escape from some supposed danger. Under such conditions the terns, which are not so protectively colored as the skimmers, take time to run to a near-by corner or shadow before squatting, while a skimmer crouches instantly, and with two or three sidewise flicks of feet and legs almost buries himself.

The act of pecking is instinctive to a certain extent, but is acquired very slowly and, at first, imperfectly, in this way. By imitation it is learned quickly and is performed successfully within a few minutes.

The art of flying is wholly instinctive, all the species learning the use of their wings as soon as the primaries are large enough to support them.

The characteristic method of feeding, which has given their name to the skimmers, begins to be apparent after three or four weeks, the birds then taking a piece of fish from one's hand with a quick run past, holding the bill so low that it almost touches the ground. A skimmer two months old 'skimmed' often through a small pool of water in its enclosure.

At first the young skimmers are very weak and helpless and are not able to compete with terns of the same age in getting their share of fish, but after about forty days they obtain the upper hand and soon become so aggressive and domineering that they have to be separated and confined with individuals of their own species.

One interesting result attained is the successful establishing of a colony of laughing gulls in the zoological park. These birds require three years to assume the adult breeding plumage, and for two seasons some twenty-five birds have made their home in the park, without the loss of an individual. Several of their number migrated south in the fall of 1903 and returned the following spring. Thus the wonderful homing instinct of these birds has been controlled and extended by bringing the eggs or the very young birds to a new locality, and though the park individuals doubtless mingled with others of their kind, possibly even associating with their actual parents, yet the *Heim Liebe* overcame all other attractions.

With the erection of the two new bird houses now almost completed, opportunity for experiment and observation upon the collections of the society will be undertaken, on a scale of elaborateness and thoroughness hitherto impossible.

C. WILLIAM BEEBE.
NEW YORK ZOOLOGICAL SOCIETY.

HEIGHT MEASUREMENTS OF THE AFRICAN PYGMIES.

Apropos of the presence of the group of African pygmies from the Wissmann Falls region in the exhibit of the department of anthropology at the World's Fair, it may be pertinent to summarize the height measurements given by various modern travelers who have recorded their observations. Unfortunately, the circumstances under which some of these observations were made were not con-

Observer.	Time.	Place.	No. Adult Males.	Height, Average.
Du Chaillu	1863	1° N. 11° E.	1 young man.	4 ft. 6 in.
Schweinfurth	1869	1 N. 23 E.	6	4 ft. 10 in.
Wissmann and Wolff	1886	5 S. 24 E.	Not given.	4 ft. 7 in.
Stanley	1888	1 N. 29 E.	1	4 ft. 6 in.
Johnston	about 1900	1 N. 29 E.	Not given.	4 ft. 9 in.
Belgian Officers	about 1900	1 N. 29 E.	5	4 ft. 8 in.
Verner	1897	6 S. 22 E.	50	4 ft. 8 in.
Verner	April, 1904	6 S. 22 E.	15	4 ft. 10 in.
Verner	Aug., 1904	St. Louis.	5	4 ft. $10\frac{3}{10}$ in.

ducive to ideal research, and the records made are not entirely satisfactory for purposes of comparative anthropometry. For example, among those measured by Du Chaillu there was only one adult male, and he was mentioned only as a 'young man,' a term somewhat indefinite. Other observers did not specify the approximate age, while others did not mention the number given. This list has been compiled from the writings of the authors themselves.

In the writer's first description of these people in 1897, there occurs a mistake made in the conversion of the metric system to English measure. The average of all the observations is four feet eight and two ninths inches, about one foot shorter than the height of the average normal man.

S. P. Verner.

St. Louis,
September 12, 1904.

CURRENT NOTES ON METEOROLOGY.

THE TEACHING OF METEOROLOGY.

In connection with the third convention of weather bureau officials, held at Peoria, Ill., September 20–22, a circular was sent out in June to all persons who were believed to be engaged or interested in the teaching of meteorology in universities and other institutions of learning. The object of this inquiry was to obtain information regarding the number and character of courses that are now given; the demand for such courses; the didactic value of meteorology as a course of mental training, and the commercial or pecuniary value of meteorology as a professional career. The replies to this circular have been printed by the weather bureau, and are not encouraging. The state of things as evidenced by the replies received from teachers and others in all parts of the country may be briefly summarized as follows: At several of the universities and colleges courses in elementary meteorology are given, usually by instructors whose main business is along other lines of work. In some cases such courses have been given and then discontinued. Weather bureau officials in many places are doing their best, in a most praiseworthy manner, to stimulate interest in the study. Scattering schools, here and there, where the physiography teachers have a special interest in meteorology, pay some attention to systematic work in the subject. But as a whole, there is a very evident lack of proper instruction in meteorology in the country as a whole, from schools to universities. There is but one university (Harvard) where meteorology may be taken as a subject for admission.

WEATHER FORECASTS FROM THE HUMMING OF WIRES.

F. Bock, of Babenhausen (Hesse), has for some time been carrying on observations of the humming of telegraph and telephone wires. As a result of this study, he has been able to lay down certain rules for forecasting coming weather conditions. These rules, set forth in *Das Wetter* for August, make it possible to forecast the local weather conditions for the current and the succeeding days. The observations of the humming should be made two or three times daily, preferably at about 11 A.M. and 6 P.M., and if the official forecast for the general district in which the station is situated is taken as an additional help, the local forecasts based on the humming have been found to be very successful.

MONTHLY WEATHER REVIEW.

In the *Monthly Weather Review* for June (dated August 19) Professor F. H. Bigelow continues his 'Studies on the General Circulation of the Atmosphere of the Sun and of the Earth' in a contribution (No. VII.) entitled 'The Average Monthly Vectors of the General Circulation in the United States.' Dr. W. N. Shaw's paper 'On the General Circulation of the Atmosphere in Middle and Higher Latitudes,' read June 2 before the Royal Society, is reprinted. Gen. H. L. Abbot, U. S. A., discusses 'Hourly Climatic Records on the Isthmus of Panama.' Father Algué, in charge of the Philippine Weather Bureau station at the World's Fair, describes 'The First Electric Storm Recorded Automatically in St. Louis, Mo.' Professor Abbe considers 'Temperatures in the Upper Atmosphere,' 'Auroras and Thunderstorms,' 'Meteorology in Austria'

and 'Meteorology in the Colleges and Universities.'

INDIAN METEOROLOGY.

THE opening address in the subsection on cosmical physics at the recent British Association meeting in Cambridge was by Sir John Eliot, late meteorological reporter to the government of India. Sir John Eliot's address is published in full in *Nature* for August 25, and deals with Indian meteorology in relation to the general meteorology of the Indo-oceanic region, and with the abnormal features of the meteorology of that area for the unique period 1892–1902. It also suggests a coordination of the meteorological observations of the British Empire, and the creation of a central office for the investigation of problems of general meteorology.

R. DEC. WARD.

SCIENTIFIC NOTES AND NEWS.

THE last issue of the program of the International Congress of Arts and Science publishes the names of 301 speakers, of whom 64 are not teachers in universities or other institutions of learning, the rest being distributed as follows, only those universities represented by three or more being given:

23: Columbia.
22: Chicago.
21: Harvard.
11: California.
10: Berlin, Paris, Yale.
8: Cornell.
7: Johns Hopkins.
6: Vienna.
5: Cambridge, Wisconsin.
4: Heidelberg, Leipzig, Massachusetts Institute of Technology, Stanford, Michigan, Nebraska.
3: McGill, Tokio.

Fifty-seven institutions were represented by one or two speakers.

PROFESSOR ROBERT KOCH has recently returned from Detmond, where he was engaged in investigating an outbreak of typhoid fever for the German government, and has since been at Paris, where he was entertained by the Pasteur Institute. In the course of the winter he will proceed to German East Africa in order to continue those studies of tropical and other diseases which he had not completed during his recent visit to Rhodesia. In particular he will continue to investigate the part played by ticks in conveying the infection of various cattle diseases.

SIR WILLIAM RAMSAY was entertained by the Physical Science Department of the Brooklyn Institute of Arts and Sciences at a banquet in Union League Club, on the evening of October 24. On the morning of the same day he spoke to the students of the College of Arts and Engineering of the Brooklyn Polytechnic Institute.

DR. JAMES WARD, professor of moral philosophy and logic, at Cambridge University, who gave a course of lectures at the University of California and one of the addresses at the International Congress of Arts and Science, has returned to England after giving addresses and being entertained at Princeton, Johns Hopkins, Wesleyan, Cornell and Columbia Universities.

COURSES of Lowell lectures are being given by Principal C. Lloyd Morgan, of University College, Bristol, on 'The Interpretation of Nature,' and by Dr. Pierre Janet, of the Collège de France, on 'Hypnotism and Allied Phenomena.'

DR. EWALD HERING, professor of physiology at the German University of Prague, has recently celebrated his seventieth birthday.

PROFESSOR I. C. RUSSELL, of the University of Michigan, has been engaged the past summer in examining the surface geology of the Northern part of Lake Michigan.

MR. G. V. NASH, of the New York Botanical Garden, accompanied by Mr. Taylor, sailed from New York to Inagua, on October 5, for the purpose of continuing the work on the flora of the Bahamas. Dr. H. H. Rusby returned from Kew in September, having spent several weeks at work in the herbarium of that institution and in the British Museum, making investigations on the flora of Colombia, South America. Dr. M. A. Howe has also returned from an extensive tour among European herbaria. Visits were made to the herbaria at Kew, British Museum, Lund,

Upsala, Copenhagen, Paris and elsewhere for the purpose of studying types of algæ. A large number of photographs and notes on critical forms were secured.

The government of India has created an imperial sanitary commissioner, separate and independent from the director-generalship of the Indian Medical Service, as the first step towards the creation of a new sanitary department. Major J. T. W. Leslie, I.M.S., has been transferred from the secretaryship of the director-general to the new appointment.

Dr. Eugene Albrecht, of Munich, has been appointed director of the Senckenberg Institute of Pathology and Anatomy at Frankfurt-a-M.

The opening paper of the approaching session of the Royal Colonial Institute, London, will be read by Dr. G. R. Parkin. It will embody the results and observations of his recent tour of the Empire as organizer of the Cecil Rhodes scholarships scheme.

A course of lectures on evolution in the vegetable kingdom was given at Gresham College, London, by Dr. E. Symes Thompson, during the first part of the present month.

Dr. A. C. Lane, state geologist of Michigan, will give a series of lectures on economic geology at the University of Michigan during the second semester.

Professor D. Clifford Allbutt, of Cambridge, made one of the addresses on the occasion of the unveiling of the tablet in memory of Dr. Jesse William Lazear, in the new surgical building of the Johns Hopkins Hospital.

We regret to record the death of Dr. Hermann Kortum, professor of mathematics at the University of Bonn, at the age of 68 years.

At the recent Cambridge meeting of the British Association, the newly established Psychological Society held a special meeting in conjunction with the section of physiology. The section of physiology is now definitely to include in its title the two studies of physiology and experimental psychology.

The next meeting of the Society of German men of Science and Physicians will be held at Meran, Austria, under the presidency Dr. Franz von Winckel, professor of gynecology at the University of Munich.

The St. Petersburg Institute of Experimental Medicine has sent an expedition to the shores of the Black Sea to inquire into the prevalence of malaria, especially in the neighborhood of Gagory.

According to Reuter's agency the Duke of Orleans has applied to the Norwegian government for permission to hire Dr. Nansen's ship, the *Fram,* for the purpose of an Arctic expedition next year. The *Aftenposten* learns that the admiralty will propose as conditions of the arrangement that Otto Sverdrup shall captain the ship, that the plan for the expedition shall be sanctioned by the Norwegian authorities, and that the University of Christiania shall have the benefit, at least in part, of the scientific results of the expedition.

M. Léon Cléry has bequeathed $20,000 to the French Society for the Protection of Animals, the interest to be awarded annually to the schoolmaster or schoolmistress who shall have done most to cultivate among his or her pupils the spirit of compassion for their dumb fellow-creatures.

The telescopes and general apparatus used for the production of the late Dr. Isaac Roberts's celestial photographs are to be sold, with the observatory house and grounds, situated 800 feet above sea-level. The telescopes and apparatus may be sold separately. For particulars Mr. W. S. Franks, F.R.A.S., Starfield, Crowborough, Sussex, may be addressed.

Dr. Buchan, the secretary of the Scottish Meteorological Society, has sent to the London *Times* the following communication, dated Hamburg, September 30: "In acknowledging the much esteemed letter of the 23d of this month the German Seewarte take occasion to express their great regret at the closure of the important observatories on Ben Nevis and at Fort William. The stoppage of the observatory on the summit so exceptionally situated, and the low level station, will altogether be found to be a heavy loss to meteorological investigation. In view of the great importance of these observatories we confidently in-

dulge in the hope that we have only to do with a temporary interruption. As, similarly, the station on Sonnblick, in Austria, is rendered possible only through the cooperation of the Sonnblick Society, which places private means at the disposal of the observatory, so also may the observatory on Ben Nevis be kept in operation. The German Seewarte has the honor to express its best thanks for the very regular receipt of weather telegrams, and, in view of the outstanding results, to express the hope that these telegrams, whose loss will be found so heavy, will again appear at no distant date.—Herz."

ON January 1 the Abbasia Observatory was transferred to its new site at Helwan, about fourteen miles south of Cairo. The site is on the desert plateau above the town, and the buildings have an open view across the desert to the north, east and south, while on the west is the Nile valley. A complete meteorological equipment and time-signal arrangement has been installed, and there is a transit house and an equatorial house, while a house for magnetic self-registering instruments is being constructed. The transit pillar is in lat. 29° 51′ 33″.5 N., long. 31° 20′ 30″.2 E., the latter value depending on that of the Venus station in the Mokattam hills (31° 16′ 33″.6 E.). The altitude of the cistern of the barometer above mean sea-level at Alexandria is 377.4 feet.

THE Bureau of Animal Industry of the U. S. Department of Agriculture has assigned one thousand dollars of the recent congressional appropriation for experiments in animal breeding and feeding to the cooperative investigations with the respiration calorimeter now in progress at the Pennsylvania Station. Plans have been made for an investigation upon the influence of age and individuality upon the metabolism of cattle and two full-blood yearling steers, one an Aberdeen Angus and one a Jersey, have been purchased for use in the investigation. Careful records will be kept of the food consumed and the rate of growth of these animals. In addition, occasional digestion experiments will be made with them to determine the assimilability of the food and the actual production of lean meat; while each winter there will be made, with the aid of the respiration calorimeter, scientifically accurate determinations of the actual gains of lean meat and fat respectively. It is expected to continue the experiment until the animals are mature. In this way it is hoped to secure more exact knowledge as to the actual points of difference in the feeding qualities of the dairy and beef types of cattle, while at the same time valuable data will be obtained as to the nature and economy of the growth made at different ages.

WE learn from the London *Times* that a new society has been formed, called the Ethological Society. The object of the society is the study of human nature, not through one department of science, but taking from all the different branches the most practical and useful to arrive at a knowledge of the intellect and character of man and the laws which govern their manifestation, and collecting all material that is available and applying it to the preservation of the mental health of the individual, to education, moral reform, and the solution of various sociological problems. The society will hold a series of *conversazioni,* at which authorities will be invited to deliver lectures on the relation of their branch of art, science or philosophy to human nature. The president is Dr. Bernard Hollander, and the preliminary list of vice-presidents includes the names of Mr. Oscar Browning, Dr. Stanton Coit, Mr. John Churton Collins, Mr. W. L. Courtney, Mr. Jerome K. Jerome, Mr. Henry Arthur Jones, Mr. George R. Sims, Dr. Alfred Russel Wallace, F.R.S., Mrs. St. Leger Harrison, Miss L. M. Faithfull and Mr. George Meredith. The inaugural meeting and first *conversazione* will be held at the Galleries of the Royal Society of British Artists, Suffolk Street, Pall-mall, S. W., on Wednesday, November 16, at 8:30 P.M., when the president will deliver an address.

THE University of London has issued its list of university extension courses, some seventy in all at various centers, for the coming session. 'The Evolution of Mankind as seen in the Child and the Race' will be given at University College on Monday evenings.

The first year's work will consist of five lectures by Dr. Chalmers Mitchell on general biology, to be followed by ten lectures on anthropology by Dr. Westermarck, and ten on animal psychology by Professor Lloyd Morgan. Among other courses will be 'Social and Industrial England,' at Ealing and Erith, by Dr. Holland Rose; 'Japan and China,' by Mr. Ernest Foxwell, at Streatham and Sutton. Professor Vivian Lewes will lecture at Balham, New-cross and Rotherhithe, on 'The Chemistry of Air, Fire and Water'; Dr. Fison, on 'Great Discoveries in Science,' at Gresham College; Professor Bottomley, at Hammersmith, on 'Recent Researches on Bacteria in Relation to Life'; Mr. Alfred Milnes, in 'Economics,' at the Regent Street Polytechnic, the City of London College and Battersea; and Mr. Chisholm and Mr. Andrews, at various centers on 'The Aims and Methods of Geographical Study.' Most of the courses will be in the evening.

UNIVERSITY AND EDUCATIONAL NEWS.

Lord Rendel, president of the University College of South Wales, Aberystwith, has for the fifth time forwarded a cheque for £1,000 to the registrar of the college, £750 being assigned to the college for the increase of the stipends of the staff and £250 to the county schools of Cardiganshire, Merionethshire and Montgomeryshire for exhibitions and bursaries.

A department of experimental psychology has been established in the Western University of Pennsylvania, under the charge of Edmund B. Huey, A.B. (Lafayette), Ph.D. (Clark). Two good-sized rooms and a dark-room are being fitted up for the new department, and an appropriation has been made to meet the initial needs for apparatus and books.

The registration at the University of Michigan has reached 4,100, the largest in its history.

According to the *Educational Times*, the buildings of the Metallurgical Department of the University of Birmingham have been opened. They form only a small portion of the scheme of building at present being undertaken at Bournbrook, which will not be completed for two or three years; but the cramped state of the present university buildings in Edmund Street necessitated the pushing forward of the Metallurgical Department, which, in a large iron center like Birmingham, is one of the most important connected with the university. All the plant and machinery are of full working size. For instance, there is a two-ton Siemens furnace, with all the latest equipment necessary for a commercial undertaking, and capable, if necessary, of a large weekly output of steel. It is not, however, intended to produce steel for sale; the object is rather to familiarize the students with the various causes, chemical and other, which affect the quality of steel. Another portion of the department will be devoted to metals other than iron and steel.

Professor F. H. H. Calhoun, Ph.D., a graduate of the University of Chicago, was recently elected professor of geology and mineralogy in Clemson Agricultural College of South Carolina. Dr. Calhoun has been engaged in practical work for the United States Geological Survey out west during the past year or so, and since his arrival at Clemson College, he has planned some important geological work in South Carolina. Dr. G. E. Nesom has resigned the professorship of veterinary science and the position of state veterinarian to accept the position of assistant commissioner of agriculture in the Philippine Islands. Dr. Nesom has been connected with the college for a number of years, and he has done valuable work for the State of South Carolina. Dr. Louis A. Klein, who is connected with the United States Bureau of Animal Industry, was recently elected to succeed Dr. Nesom in Clemson Agricultural College. Dr. Klein will report for duty on November 1. He is a graduate of the University of Pennsylvania in the veterinary department.

Dr. Emil Wiechert, who recently declined a call to the University of Königsberg, has been promoted to a full professorship of mathematical physics in the University of Göttingen.

Dr. Erich Kayser, district geologist at Berlin, has been appointed full professor and director of the Mineralogical Institute at the University of Giessen.